About this region there is a system of arched commissural fibres, the fibræ arcuatæ. They seem to be connected with the crura cerebelli and medullam. They occur not only through the external part of the ventral surface, but also through the central portions.

The acusticus and glossopharyngeal arise from the grey substance on the floor of the fourth ventricle.

The vagus arises from a series of rounded tuberosities situated on the side of the floor of the fourth ventricle; each root arises from a separate tuberosity.

The spinal nerves arise by dorsal and ventral roots; the latter from the ventral horn of grey substance. The former pass obliquely into the interior of the cord and there divide into two bundles; one bundle from the anterior part of the root is directed backward, the other bundle from the posterior part of the root is directed forward. They pass over the next nerve both in front and behind, and join the lateral columns of the cord. This arrangement was first described by Stieda.*

January 14, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Action of Sunlight on Micro-organisms, &c., with a Demonstration of the Influence of Diffused Light." By ARTHUR DOWNES, M.D. Communicated by Professor MARSHALL, F.R.S. Received December 9, 1885.

Eight years ago, conjointly with my friend Mr. Blunt, I communicated to the Royal Society an account of an experimental inquiry into the action of sunlight on the micro-organisms of putrefaction and decay.†

We adduced evidence, conclusive in our opinion, that the solar rays were very hostile to these lowly forms of life; so much so that under favourable conditions bright sunlight, sufficiently prolonged, would altogether prevent their appearance in fluids which, under

* "Zeitsch. f. Wiss. Zoologie," Bd. 23, 1873.

† "Proc. Roy. Soc.," vol. 26, p. 488, and vol. 28, p. 199.

similar conditions of temperature and the like, but screened from light, swarmed in a very few days with countless saprophytes.

By means of suitable absorptive media, we learned that the most active rays were those of the more refrangible end of the spectrum.

Seeking an explanation of the facts thus observed, we proceeded in the first instance by analogy.

We found that light had an oxidising action on many organic substances of comparatively simple composition, and we demonstrated that, in the presence of free oxygen, the molecule of oxalic acid might be speedily and entirely resolved into water and carbonic acid by the action of light, more especially by those rays to which I have already referred.

Proceeding to more complex substances, we applied the same method to one of those singular bodies, the so-called soluble or indirect ferments.

In less than a month the properties, and, inferentially, the substance, of the invertive diastase of yeast were destroyed by light.

Once more we found that we had to deal with an oxidation. Finally, our inference that the action of sunlight on the organisms of our cultures would likewise prove to be an oxidation was confirmed by direct experiments, in which the effect varied in proportion to the amount of free oxygen present.

As yet no one has repeated these investigations in their entirety, but sufficient confirmatory evidence has accumulated to justify me, I think, in briefly placing the case before the Society as it now stands, with one or two additional observations of my own, and to afford me an opportunity of replying to one or two points of criticism.

The earliest corroboration of our work came out on the reading of our first memoir on this subject.

Mr. Warington had that same evening, in a paper to the Chemical Society, notified, but was unable to explain, the inhibitory action of light on the process of nitrification. Our experiments at once suggested to Dr. Gilbert the interpretation, since confirmed by several observers,* that light was inimical to the nitrifying ferment.

Gladstone and Tribe ("Journ. Chem. Soc.," August, 1883) found that light was detrimental to the development of fungoid growths in solutions of cane-sugar exposed to atmospheric air.† Tyndall re-

* Soyka, in "Zeit. f. Biol.," 1878; Schloesing and Muntz, "Journ. of Chem. Soc." (Abstr.), April, 1880.

† It is right to state that van Tieghem in his investigations on the organisms appearing in olive oil ("Bull. Soc. Bot.," xxviii, p. 186), found that *Penicillium glaucum* developed in oil at the most illuminated spots. I have not been able to see the original paper, and am therefore not acquainted with the conditions of the experiment, especially as regards the nature of the illumination, and the access of free oxygen.

ported to the British Association in 1881 the results which he had obtained in exposing flasks of animal and vegetable infusions to the influence of an Alpine sun. Corresponding flasks shaded from the light became turbid in twenty-four hours, "while thrice this time left the exposed ones without sensible damage to their transparency." He satisfied himself that this was not due to differences of temperature. The amount of insolation was insufficient, however, to permanently sterilise the cultures after removal to a warm kitchen.

Confirmatory evidence also, invested with a special value by the author's great experience of saprophytic life, has more recently been adduced by Professor E. Duclaux.* With the usual precautions he introduced into a flat-bottomed Pasteur flask a drop of pure culture of the organism to be studied, and dried it under a desiccator. After the desired period of exposure to the sun, the flask was charged with sterilised nutrient liquid, *of a kind specially suitable to the development of the particular organism*, and placed at a favourable temperature in an incubator. Corresponding flasks were kept in the dark.

He finds that the general rule, that the spores of these organisms resist adverse influences better than their vegetative forms, holds good in regard to the effects of light. This accords with our own observations on the insolation of germs in water.† M. Duclaux, however, reserves for a future memoir his conclusions on the insolation of the *micrococci*—among which the formation of spores is not certainly known.

The very hardy spores of the *Bacillus*, to which he has given the name of *Tyrothrix filiformis*,‡ were destroyed by thirty-five days' exposure to an autumnal sun. *T. geniculatus* was more resistant, but showed signs of commencing enfeeblement. *T. scaber* was only retarded in development by insolation during the month of August, 1884, but a further exposure to the end of September—a not very fine month—sterilised two flasks out of four.

Similar spores had survived for three years in the dark. Like Professor Tyndall, he was satisfied that these results were not effects of temperature; his insolated flasks scarcely reached any point higher than their fellows kept in darkness in an incubator.

He concluded, also, from further experiments, that the injurious influence of light here manifested was probably an affair of oxidation.

M. Duclaux very rightly insists on the importance of careful adaptation of the nutrient liquids to the organisms operated upon, observing that, otherwise, spores might be regarded as dead which had only, perhaps, been enfeebled. "C'est là l'objection," he continues,

* "Ann. de Chim. et de Phys." 6e ser., t. v, Mai, 1885.

† "Proc. Roy. Soc.," vol. 28, pp. 203–4.

‡ "Études sur la lait," "Ann. de l'Instit. Agronomique," 1879–80.

"qu'on peut adresser aux expériences très bien conduites, du reste, et très intéressantes de M. A. Downes." ("Proc. Roy. Soc.," vol. 26, p. 488, 1877.)

As regards the earlier of our memoirs, to which M. Duclaux refers, this criticism is just. At that date knowledge of revivification of germs in different media was neither so generally diffused nor so precise as it now is. This advance we owe not least to Professor Duclaux himself. Accordingly, in our first experiments we regarded non-appearance of life in our insolated tubes of Pasteur solution, or of urine, as proof of destruction of the organisms which they had originally contained. In our second and more complete memoir, however, we reserved our opinion on this point.

But it was an essential principle of the method on which we worked, and the key to our success, that our nutrient fluids should be sufficiently resistant to bacterial growth to hinder the development of organisms, through the night, or during cloudy days, from outrunning the inhibitory effects of insolation. It would probably, for example, not often be possible to secure in England the results which Tyndall obtained on the Alps with apparently considerable bulks of very putrescible materials. Pasteur solution and the like are at their best but limited media of nutrition;* yet under special circumstances, as, for example, in the demonstration of the action of diffused daylight given below, it is necessary to largely increase their resistance to decomposition.

Moreover, the question whether the germs in our solutions were or were not actually dead, does not affect the truth of our induction. I cannot put this more pithily than has Professor Duclaux himself, in a very courteous communication with which he has favoured me. "You have clearly shown," he says, "that an insolated germ is a sick, sometimes very sick, germ; death is but a step further."†

It was an *à priori* probability that micro-organisms should vary considerably in their powers of resistance to the oxidising influences of light. In our previous papers, indeed, we gave examples of this in the frequent survival over *Bacteria* of some less sensitive form of *Saccharomyces* or *Mucedo*.

I have lately met with an instance which may be worth recording, as it enabled me to isolate a *Bacterium* of which I can find no previous description. In each of a number of thickish glass tubes I had sealed up 3 c.c. of distilled water, together with a small bulb containing an

* For prolonged insolation I have for another reason quite abandoned their use. They are liable to take on a brown coloration in sunshine, and I have had, in consequence, to abandon a laborious series of experiments.

† M. Duclaux tells me that he has also confirmed our observation that the diastases are destroyed by sunlight. He operated on the *diastase présure*, the coagulating principle of rennet.

equal quantity of carefully sterilised peptone solution (double strength). Some of these tubes were insolated on an outside shelf facing south; others were incased in laminated lead alongside.

After the desired period of insolation, the bulbs were broken by a jerk, and the tubes, now containing 6 c.c. of peptone solution of ordinary strength (2 per cent.), were removed to a warm cupboard kept at about 20° C. By a week's exposure, May 29—June 5, bacterial development was already retarded (sixty hours as compared with twenty-four). After insolation for nearly four weeks, May 29—June 24, ordinary bacterial development appeared in two incased tubes in thirty-six hours. In two insolated tubes, at the same date, nothing was seen till the fourth day, when small flakes began to form, and by August 3 had settled into a dirty-white collection, leaving the supernatant liquid clear, presenting a notable contrast to the uniform turbidity of the incased.

These flakes were found to consist of compact spherical or cylindrical nodulated masses of zooglœa. They closely resembled in general appearance the *Ascococcus Billrothii* or *Ascobacteria* of v. Tieghem, but I was utterly unable to demonstrate the gelatinous envelopment from which those organisms take their name.

On teasing out a portion the colony was found to consist of closely felted small rods, motile when freed from the mass, about $0{\cdot}6\mu$ diameter, and $2{\cdot}0$—$3{\cdot}0\mu$ long.

On September 28, after four months' exposure, the remaining tubes, three insolated and one incased, were taken in. Nine days elapsed before the latter became hazy with *Bacteria*. In eleven days one of the insolated contained flakes, such as I have above described. In a day or two later similar flakes formed in another of these tubes. The third insolated tube subsequently broke down with a scanty development of *Bacteria*, not distinguishable from the kinds found in the incased.*

It is evident that the zooglœa-lump-forming Bacterium was especially resistant to sunlight, and so became isolated in almost pure cultures in four-fifths of the tubes insolated for a month and upwards.

I wish now to direct attention to the fact that the tubes of the experiment which I have just described, were exposed repeatedly to considerable elevations of temperature. The meteorology of Greenwich may be taken as sufficiently identical with that of Chelmsford,

* This experiment—an insolation of germs in water only—might be regarded, and possibly rightly so, as confirmatory of what we have previously written on the resistance of germinal matter in a fluid devoid of nutrient material. But it should be remembered that the supply of free oxygen was necessarily limited in these sealed tubes, being rather less than 5 c.c. in each, and I am unable at present to say whether this amount would be sufficient to oxidise the germs ordinarily present in 3 c.c. of distilled water.

where the investigation was made.* At the Royal Observatory the means of the maxima in the sun's rays were :—

June........	126·1° F. (52·3° C.).
July........	143·0 F. (61·6 C.).
August......	129·2 F. (54·0 C.).

It cannot be doubted that these tubes were often exposed to a temperature of 140° F. (60° C.), and on at least one occasion (July 27) to 160° F. (71° C.).

The incased tubes had for radiant heat a somewhat greater absorptive power than the bare glass of the insolated. For temperatures below 100° F. (38° C.) this difference was comparatively slight; at 100° F. it was 4·5° F. (2·5° C.).

It is certain, therefore, that any deleterious influence of heat should tell more on the incased than on the insolated. Yet at the end of four months *Bacteria* appeared, retarded in development, it is true, but still morphologically identical with the forms originally found in similar solutions.

I lay stress on these facts, because an Australian observer has declared† that we, and Professor Tyndall with us, have mistaken effects of heat for supposed effects of actinism.‡

But Dr. Jamieson's paper in abstract has gone the usual round of German year-books and periscopic notes of English journals, until an impression has arisen that there is no satisfactory evidence of injurious influence of light on micro-organisms. I trust, therefore, to be permitted a few words in reply.

Dr. Jamieson insolated Cohn's solution in phials, and found that in a short space of time bacterial development might thereby be entirely prevented. But in some of his experiments he thought that he succeeded better in hot weather than in cool, and he failed to produce any effect in diffused light. He asked himself, therefore, whether the results he had noted might not have been, after all, due to heat.

This was a very legitimate question, but, instead of solving it by direct observation, he unfortunately recalled to mind experiments§ in which *B. termo* had apparently been killed by seven days' exposure to 45° C. (113° F.), by fourteen hours at 47° C. (116·6° F.), by three or

* Greenwich.......... *Lat.* 51·28 N. *Long.* 0·00.
Chelmsford......... „ 51·44 N. „ 0·28 E.

† Royal Society of Victoria. June, 1882.

‡ We cannot dispense with some word such as this to connote energy not necessarily coincident with effects either of solar heat or luminosity. In using it above, I go a little further than Professor Tyndall, who has not, I believe, yet given any opinion as to the form of radiant energy, except that it is not *heat*, which he found to hinder bacterial development.

§ "Eidam. Beit. zur Biol. der Pfl.," Heft iii, p. 208.

four hours at 50—52° C. (122—125·6° F.), and by one hour at 60° C. (140° F.). He argued that some of these temperatures were commonly attained in the sun's rays in Australia, and even in England; he thought that 125° F. (51·6° C.) would occasionally be experienced for a few hours, and he concluded that his, our own, and Tyndall's results were all effects of temperature.

The argument is fallacious. It is true that some organisms in certain stages of their development may be destroyed by lower degrees of heat than is commonly supposed. Duclaux has given an instance in which forty-eight hours at 38° C. (100·4° F.) was fatal to some very old yeast globules.

But even of these it is true only in their vegetative forms; their spores (and the spore-form is doubtless that in which the micro-organisms originally existed in our nutrient liquids) resist elevations of heat far surpassing anything noted above. Were it otherwise bacterial life would probably soon cease to be. It is obviously incorrect to argue that, because some organisms in some phase of their existence are destroyed by moderate heat, all organisms, in all phases and under all conditions, are so too, and any inference drawn from such reasoning must be rejected.

Moreover, I have already shown that the laminated lead used in our experiments absorbed radiant heat in greater degree than the bare glass, and consequently that our incased tubes would be more affected by solar heat than our insolated. And I need only refer to our previous demonstration, that the greatest effect on micro-organisms is produced by those rays which occupy the cooler portion of the spectrum.

Dr. Jamieson failed with diffused light. His failure was due to his method of experimenting.*

I have already said that an essential element of success is to apportion the natural resistance of the cultivation liquids to the amount of light available. Bacterial development once started usually outruns even direct sunlight, both by increasing the opacity of the fluid, and by quickly reducing the amount of oxygen. Naturally diffused light would be far slower in action than the direct solar ray, and we must select either very cool weather for the experiment, or must choose solutions of considerable resistance.

Keeping this principle in view, I have found it easy to show that diffused light possesses properties differing only in degree from those which we have demonstrated in regard to direct sunlight. I have made a number of experiments in which ordinary thin test-tubes plugged with cotton-wool were placed in a box (20·5 cm. cube) lined

* Moreover, he seems to have placed his bottles inside a window. The absorptive power of glass has always prevented me from succeeding in such circumstances.

with white paper, having one side open, and tilted at such an angle as to receive diffused light straight from the white clouds of the northern sky. By no possibility could direct sunlight find an entrance. In the box were placed maximum and minimum thermometers, each pair with bulbs respectively incased or left bare.

In September, 1883, using Cohn's solution five times the ordinary strength, in five days, four out of six incased tubes were noted as "turbid," and the other two as "hazy," with bacteria. The exposed tubes were recorded "beautifully clear." At the end of two days more the latter were still clear, but in each were specks of mycelium. The survival of mycelial growth over bacterial has already been alluded to in the present paper, and is seen in Dr. Jamieson's own experiments.

But mycelial growth itself may be hindered by diffused light.

In March, 1884, a *slightly acid* Cohn's solution, two and a half times ordinary strength, being specially selected, I found that at the end of ten days $\frac{5}{6}$ of the incased tubes contained mycelial specks, the six exposed tubes being perfectly free. At the end of fifteen days my notes were:—"Disks of mycelium plug $\frac{5}{6}$ of incased, $\frac{2}{6}$ of exposed; small tufts of mycelium in the remaining incased tube, and in one of the exposed, three remaining exposed quite clear. The difference in appearance of the two sets is remarkable."

The means of the thermometrical readings during two periods were:—

		Encased.	Exposed.
Sept. 1883..	Max.	63·5° F. (17·5° C.)	64·0° F. (17·8° C.).
	Mim.	44·7 F. (7·0 C.)	45·0 F. (7·2 C.).
Oct. " ..	Max.	56·0 F. (13·3 C.)	57·8 F. (14·3 C.).
	Min.	35·6 F. (2·0 C.)	36·8 F. (2·6 C.).

As the incased tubes were the better absorbers, so are they now seen to be the better radiators, and conditions of temperature were accordingly slightly more adverse to development of organisms in them as compared with the exposed.

I now conclude this paper with a reference to the researches of Herr Pringsheim on chlorophyll.* I refer to them with especial gratification, as evidence of the truth of a generalisation which I had ventured to draw from our experiments.

The micro-organisms of our solutions may be regarded as examples of protoplasm in its simplest forms, but there are no grounds for supposing that this "life-stuff" should be subject to hyperoxidation by light only when it exists in a *Bacterium*, or a mycelial thread. On the contrary we have probably to deal with a general law, and, without protective developments of cell wall, or of colouring matter which

* "M. B. Akad. Wiss.," Berlin, 1879.

should filter out injurious rays, &c., living organisms could hardly endure the solar light.

Pringsheim operated on chlorophyll tissues. By means of a lens and a heliostat he concentrated upon them sunlight, from which by suitable media he had sifted out the heat rays. In a few minutes the green colouring matter was destroyed, the protoplasmic circulation arrested, the protoplasm disorganised, and the cell flaccid and inert. He found, as we had found, that the more refrangible rays were the most powerful, and he, too, concluded that he was dealing with an oxidation, for in an atmosphere of hydrogen or of carbonic acid these destructive results no longer ensued.

The experiments of Siemens* and Déhérain† also demonstrate both the destructive influence of the electric light on vegetation and the protective effect of a glass screen.

[Note.—According to an abstract in "Journal of Science," 3rd ser., vol. vii, p. 594, M. Duclaux has since published the results of his observations on six species of *micrococci*, apparently of pathogenic kind.‡

Forty days of insolation (May 4—June 13) proved sufficient to kill and less to attenuate these germs in the moist state. In a desiccated condition eight days (May 26—June 3) proved fatal; in July none resisted three days' exposure at a south window which received the sun only from nine to one o'clock each day, and where the temperature did not exceed 102° F. (39° C.). Fifteen days of July sun destroyed the micrococci in the moist state. He had not in these experiments eliminated any partial influence of temperature. January 4, 1886.—A. H. D.]

II. "Notes upon the Straining of Ships caused by Rolling." By Francis Elgar, LL.D., F.R.S.E., Professor of Naval Architecture and Marine Engineering in the University of Glasgow. Communicated by Sir E. J. Reed, F.R.S. Received December 28, 1885.

(Abstract.)

It does not appear that any serious attempt has yet been made to investigate the amounts, or even the nature, of the principal straining

* "Rep. Brit. Assoc.," 1881.

† "Journ. Chem. Soc." (Abst.), Jan., 1883. Considerations of time and space prevent me from noticing many other observations of interest in connexion with this subject; *e.g.*, of Engelmann on *Pelomyxa* ("Arch. f. Phys.," xix, 1879) and on *B. photometricum* ("J. Roy. Micr. Soc.," Abst., 1882–2), or of Stahl, "On the Arrangement of Chlorophyll Bodies in Plant-Cells" ("Bot. Zeit.," 1880).

‡ "Comptes rendus," 1885.